We warmly thank the Editor and Reviewers for thoroughly reviewing our paper. We believe that comments and suggestions have greatly improved our paper's quality, making it more effective and clearer. Please find below our point-by-point replies (red text). All page and line numbers refer to the revised version of the manuscript unless otherwise specified.

Editor

Thank you for providing your replies to the comments of the four reviewers. While most of the reports are generally positive, the reviewers also raised some critical comments and suggestions that should be addressed and could potentially strengthen the paper. I would like to invite you to revise the paper accordingly and upload a revised version that would be re-evaluated by the reviewers.
Sincerely,
Nadav Peleg

Dear Editor,
The numerous comments made us improve the paper substantially, introducing several new parts. While we reply in detail to the four reviewers below, the main changes/improvements can be summarized as follows:

1. Introduction largely revised, according to the comments of the referees;
2. New separated Discussion section, divided into two subsections (4.1 – Trend analysis and 4.2 – Mediterranean Sea warming effect on precipitation), in which, besides moving already existing discussion paragraphs, new comments were added;
3. Number of bibliographic references, due to the improvements in both the Introduction and Discussion, significantly increased and now almost equal to 150;
4. New figures (Figs. 2, 8, 13, 15) and revised figures (Figs. 1, 3-7, 10-12, 14)
5. A new Table (Table S1) and nine new figures (Figs. S1-S4, S25-S29) in the Supplementary;
6. A new analysis in which several geographical and orographic factors were analyzed to study how effectively they affect average annual precipitation and trends observed by the ground-based monitoring network (results shown in Table S1 and Fig. S2);
7. A comparison analysis between ERA5-Land and ground observations (results shown in Figs. S3 and S4);
8. An extended analysis of the main event (event no. 12), adding two new figures (Figs. 13 and 15). Furthermore, such analysis has been complemented by considering two other events (no. 6 and no. 15, Figs. S27-S29);
9. Two new experiments, both applying spectral nudging to the outer domain (D01) on the geopotential (ERA5 source) at heights above 500 m, for SST0 and SST+3 scenarios (Fig. S1);
10. A sensitivity analysis about PBL parameterization (Fig. S26);
11. A new analysis on the locations of the centers of mass of the accumulated precipitation exceeding the 95th percentile based on the lower-resolution (Domain D01) simulations (Fig. S25).

Furthermore, the whole paper was carefully re-read and checked to improve English further and adjusted to avoid vague wording.
Overall, we are confident that all the changes made contribute to strengthening and making the paper more complete.

Anonymous Referee #1

General Comments

This study investigates the evolution of heavy precipitation events in the Mediterranean basin under a changing climate, focusing on the role of sea-atmosphere-orography interactions. Using both observational data and numerical simulations, the authors assess the trends in annual and maximum precipitation, with an emphasis on the Calabrian peninsula. They also explore how sea surface temperature changes might impact precipitation patterns, particularly during intense rainy seasons. The study emphasizes the importance of high-resolution, convection-permitting models to capture key processes. While the topic is relevant and the approach is promising, several aspects of the manuscript require clarification and improvement to strengthen the overall impact and scientific contribution.

We thank the Referee for the positive feedback and for highlighting the most important aspects of the research. We acknowledge that various aspects of the analysis require further strengthening. Below, we attempt to address the suggested concerns and comments.

Introduction

This section is overall well-written and provides relevant background. However, it does not clearly address the specific research gaps this study aims to fill. The authors should explicitly state the study's novelty (for example, the use of 20 real-case events at convection-permitting scale with calibrated SST perturbations) and how it builds on existing work.

The Introduction was largely revised, according to the Referee's suggestions. Specifically, we focus on existing work on the SST effect on precipitation in the Mediterranean in LL69-78 and (new text) LL79-86. Then, we highlight the research gap (i.e., why our research is needed) in LL90-93 and point out specific novelties in LL96-97 ("verifying and updating the information about the decreasing mean/increasing variance paradox"), LL108-109 ("for the first time a set of 20 real-world events with different intensity […] and reproducing them at convection-permitting scale of 2 km"), and L115 ("specifically focused on the impact on land areas").

Data and Methods

1. This section would benefit from greater clarity and conciseness. The SST perturbation approach is not clearly explained, details on the magnitude, spatial pattern, and implementation (e.g., uniform or spatially varying changes) are missing.

The Data and Methods section was also deeply revised in all four subsections. Specifically, the SST perturbation approach is more comprehensively explained in the new subsection 2.3, LL203-212.

2. The quadrant classification based on PRCPTOT and RX1day trends is also unclear, a clearer definition or simple diagram would help.

The explanation about the quadrant classification has been improved (LL184-189). In addition, as suggested by the Referee, a diagram is shown in the new Fig. 2.

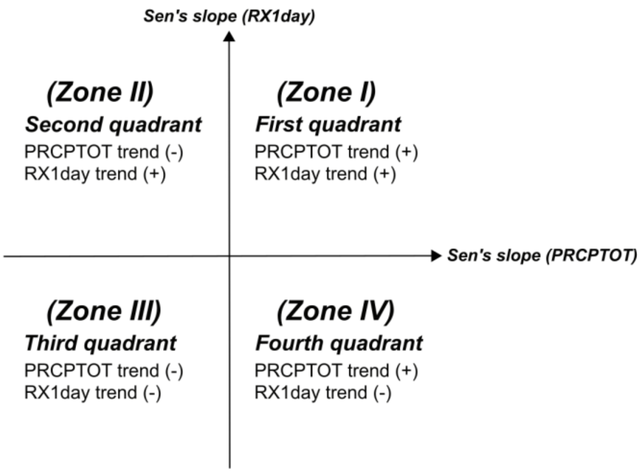

Fig. 2: Quadrant classification diagram based on Sen's slopes of PRCPTOT and RX1day.

3. Additionally, the event identification method (starting the event the day before precipitation begins) is unconventional, as most studies define event onset based on a precipitation threshold or objective criteria, further justification is needed.

We sincerely thank the Referee for this comment, which points out a wrong statement in the manuscript, 'survived' from previous versions (it was related to some spin-up issues). The new description is added in LL231-234.

Results and Discussion

Overall, the results and discussion sections offer valuable insights, but they lack clear physical explanations to support the findings.

Such as other Sections of the paper, this one was also deeply revised. In addition, according to the suggestions of the Anonymous Referee #3, the Discussion was separated from the Results.

1. In the Trend Analysis section, the authors present an analysis of observed rainfall trends in Calabria but fail to adequately link these results to the region's orographic features. While the orography is mentioned multiple times, there is no attempt to explain how it might be influencing the observed rainfall patterns, particularly in terms of total maximum rainfall. This lack of connection makes it harder to understand why different trends are observed.

A new analysis has been proposed and discussed based on the available dataset, in which geographical and orographic features like latitude (N), longitude (E), elevation, slope, aspect, and distance from the sea were analyzed to study how effectively they affect observed average annual precipitation, as well as trends of PRCPTOT and RX1day in terms of Sen's slope. The analysis is described in LL276-283, and results are shown in Table S1 and Fig. S2 (Supplementary).

2. In the Observed and Projected SST Warming section, the authors choose SSP changes of -1 and +3°C for their simulations, but the rationale behind these choices is unclear. Were these values based on region-specific data, or were they generalized from the broader Mediterranean basin? It would also be more logical for the authors to focus more on their study area (Domain 3) rather than a larger region.

While in the previous version of the paper the spatial extent of the analyzed region corresponded to the IPCC Atlas Mediterranean region, now it focuses on the external domain (D01) of the WRF simulation. This is the reason why previous Fig. 6 (now Fig. 7) has been changed, and the text was changed accordingly.

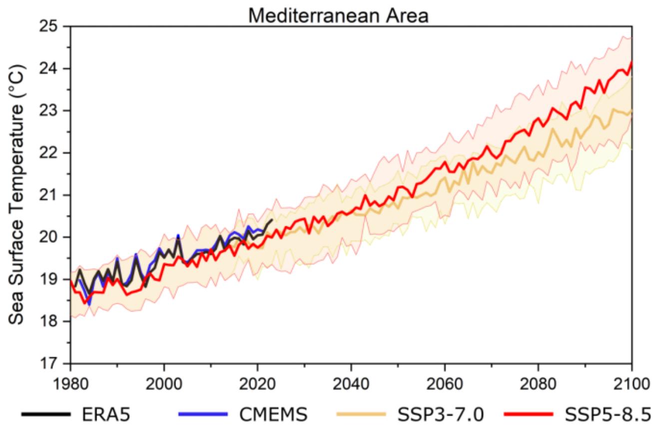

Fig. 7: Annual average SST (°C) in the external domain (D01) of the WRF simulation (please refer to the revised paper for more details).

Concerning the rationale behind the choice of the SST changes, the whole subsection 3.2 "Observed and projected SST warming" was moderately reworded. However, the main change that we believe makes our choice clearer is the new Fig. 8, in which we show the SST increase compared to 1985-2014 in the periods 2040-2069 and 2070-2098, and the observed anomalies in 2019 compared to the reference period.

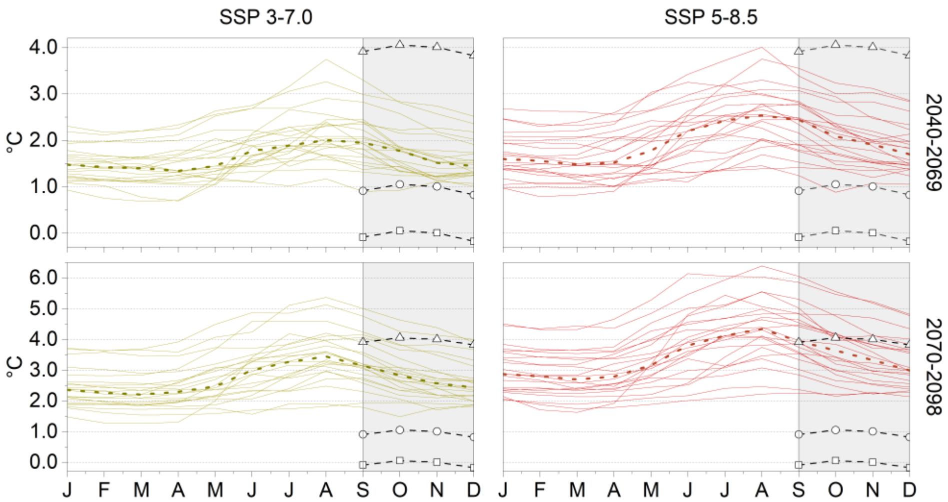

Fig. 8: SST increase compared to 1985-2014 in the periods 2040-2069 and 2070-2098, and the observed anomalies in 2019 compared to the reference period (please refer to the revised paper for more details).

3. For the WRF Simulation Evaluation, the authors chose to spatially interpolate the observations. However, it's unclear whether this is the most appropriate method for comparison. Interpolating observations can introduce uncertainty and might not accurately reflect the spatial variability of the actual observations. It would be useful for the authors to justify why they chose this method.

The choice of spatially interpolating the observations for further analysis is justified in LL153-157.

4. The Figure 12 showing the eastward shift of extreme rainfall events is visually appealing, but the authors do not explain the underlying mechanisms driving this shift. While they focus on a single event, there is no physical explanation for the observed trend, which limits the depth of the physical understanding gained from the analysis.

The underlying mechanisms driving the eastward shift of maximum precipitation accumulation are now described in more detail in LL401-407. Furthermore, a SAL analysis was added (LL408-411) to confirm the results achieved. Finally, the analysis of the single event (event no. 12) was extended (LL412-435), adding two new figures (Fig. 13 about synoptic configuration and Fig. 15 about vertically integrated water vapour flux across a specific section), and complemented with the analysis of the other two events (no. 6 and no. 15, Figs. S27-S29).

5. In the Comparison with Previous Studies section, the authors compare their findings to studies that account for global warming, rather than focusing only on SST. This comparison could lead to misleading conclusions because the underlying drivers of rainfall changes could differ between global warming and SST warming alone. More clarity on the boundaries of their analysis and comparison with relevant studies focusing on SST would enhance the discussion.

A detailed comparison with relevant studies focusing on SST has been added in LL483-498. Furthermore, the boundaries of our analysis and related limitations are further detailed in LL499-512 and in the discussion already present in the previous version of the paper (now, LL513-543).

6. Lastly, Figures 7 and 8 present an overwhelming amount of information, making them difficult to interpret. The authors might consider finding an alternative way to present these results, perhaps by simplifying the figures or breaking them down into more digestible parts.

Figs. 7 and 8 have been replaced by the new Fig. 8 (shown above).

Conclusions

1. The conclusions section lacks impact and doesn't clearly tie the study's findings together. It doesn't explain how the research advances our current understanding. While the authors summarize their results, they could better highlight the practical implications of their work. For example, they could link their findings to how the study might help predict or mitigate future storms. It would also be useful to mention the key implications of these trends for climate

adaptation or urban planning, especially in terms of how extreme precipitation affects flood risks.

We thank the Referee for their several valuable suggestions. Conclusions were revised according to the Referee’s comments. In particular, the points raised here are addressed in LL568-574 (how the research advances our current understanding) and LL575-587 (practical implications of the work).

2. Lastly, the statement that "only high-resolution, convection-permitting analyses can accurately capture key processes" is too strong and could benefit from further context.

This sentence was modified (“[…] demonstrating the added value of high-resolution, convection-permitting analyses in accurately capturing key processes unique in orographically complex regions like the one addressed“) and contextualized better (LL559-566).

Anonymous Referee #2

General Comments

The manuscript "Increasing Daily Extreme and Declining Annual Precipitation in Southern Europe: A Modeling Study on the Effects of Mediterranean Warming?" by Senatore et al. addresses an important and timely issue of the apparent paradox between increasing event (or daily) rainfall amounts despite the overall drying of the Mediterranean. While this issue was previously discussed in many studies, at least since Alpert et al. (2002), a complete answer seems to be lacking from the literature, and studies that provide pieces of this puzzle are needed.

The authors use long-term ERA5-Land data to show the context of precipitation pattern change throughout the Mediterranean (and Europe), numerous gauge records from Calabria to show these changes over the region, and high resolution WRF model simulations to investigate the impact of changing sea surface temperature on precipitation. The combination of tools and analyses provides a potentially valuable contribution to the field; however, while the study, as it is contextualised, is ambitious, it suffers from several weaknesses that make this framing exaggerated. Specifically, (a) the manuscript lacks clear articulation of the knowledge gap it seeks to address (is it precipitation increase during extremes while total precipitation decreases? Is this clearly answered in the text?); (b) the authors invest significant effort in justifying the representativeness of both the Calabria region and the specific season chosen for simulation, suggesting that these are indicative of broader Mediterranean conditions and climate change trends. While these arguments are reasonable, the paper would remain valuable even if it were framed more narrowly — as a regional case study focused on Calabria and/or a specific season. Overextending the generalization to the entire Mediterranean may risk overstating the broader applicability of the results; (c) Lastly, the authors frame the research in the intensification of extreme precipitation while overall precipitation is decreased. However, their results suggest increase in total precipitation. While these are reasonable results given the framework of increased SST only, this should be explicitly mentioned, and the authors should explain whether their study shows an exception to the general behaviour, or it only addresses a specific part of climate change effects on precipitation.

Given these comments, in my view, the manuscript should undergo major revisions before it could be published in HESS. Further comments are written below.

We thank the Referee for the generally positive feedback. We particularly appreciate their acknowledgment that, about the issue addressed, "a complete answer seems to be lacking from the literature, and studies that provide pieces of this puzzle are needed", which provides strength to the general purpose of our paper, and the potential added value of the combination of tools and analyses used. Concerning the three general comments that have arisen:

(a) The main aim of this paper is to investigate the extent to which Mediterranean Sea warming contributes to the seemingly counterintuitive increase in daily precipitation extremes in southern Europe, despite a general decline in annual precipitation. We acknowledge that this main purpose could be made clearer, and to this aim, we reshaped the Introduction section, taking up also the Referee's suggestions and comments (particularly, major comment no. 1). We firmly believe that the text contains useful insights concerning the topic, and we tried making our answer clearer.

(b) We acknowledge the recalled risk of "overextending the generalization to the entire Mediterranean", which may overstate "the broader applicability of the results." On the other hand, we do not consider it correct to overlook the representativeness of the study area in the broader context of the Mediterranean, which, as the Referee states, is supported by reasonable arguments. Therefore, in the revised version of the manuscript, we strived to balance these two aspects, defining the limits and boundaries of the results obtained more clearly.

(c) We assume that the conclusion of the Referee that our results "suggest increase in total precipitation" lies in the fact that, considering the precipitation events analyzed, the total amount is higher with SST+3 than with SST0. However, already in the precious version of the manuscript, we stated that "each event was considered as standing alone with respect to others" (previous L179) and "can be considered as isolated" (previous L347), while, among the information missing in our PGW scenario, "the expected frequency of cyclones, given the large-scale circulation dynamics changes induced by climate warming, is another relevant piece of information missing" (previous L349). Therefore, while we have proven that most of the events, individually analyzed, produce more rain under SST+3 conditions than under SST0, we can't prove a generalized increase in total precipitation because we can't provide information about the projected frequency of such events (and about other aspects). In the revised manuscript, we have included and expanded this discussion, as explained in detail in our replies to specific comments, which can be found below.

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Major comments

1) The last paragraph of the introduction provides both further motivation and background for the study (L64-67) and the task/object of the manuscript (L67-L84). In my view, the introduction lacks a clear description of the *need* for the work. After reading the first part of the introduction readers are likely familiar with some of the challenges and background related to understanding the impact of global warming on precipitation in the northwest Mediterranean. However, there is no direct statement clarifying what is not known and what is going to be resolved with this work. Please consider adding a description of the *need* and separate it from the *task* and *object*.

The Introduction was largely revised, according to the Referee's suggestions. Specifically, we focus on existing work on the SST effect on precipitation in the Mediterranean in LL69-78 and (new text) LL79-86. Then, we highlight the research gap (i.e., why our research is needed) in LL90-93 and improve the description of the object and the task in LL94-116, highlighting the peculiarities (strengths) of the analysis performed.

2) The use of EURO-CORDEX in the study is clear. However, presenting the results over the entire domain is not necessary. The authors should consider focusing this part of the results on the Mediterranean area only.

Following the Referee's comment, the study area used for the regional analysis was reduced to the Mediterranean area (30° to 50° N and 15° W to 45° E). The text was changed throughout the manuscript, especially concerning the Results subsection 3.1 ("Trend Analysis"), where we provide the new Figs. 3 and 4.

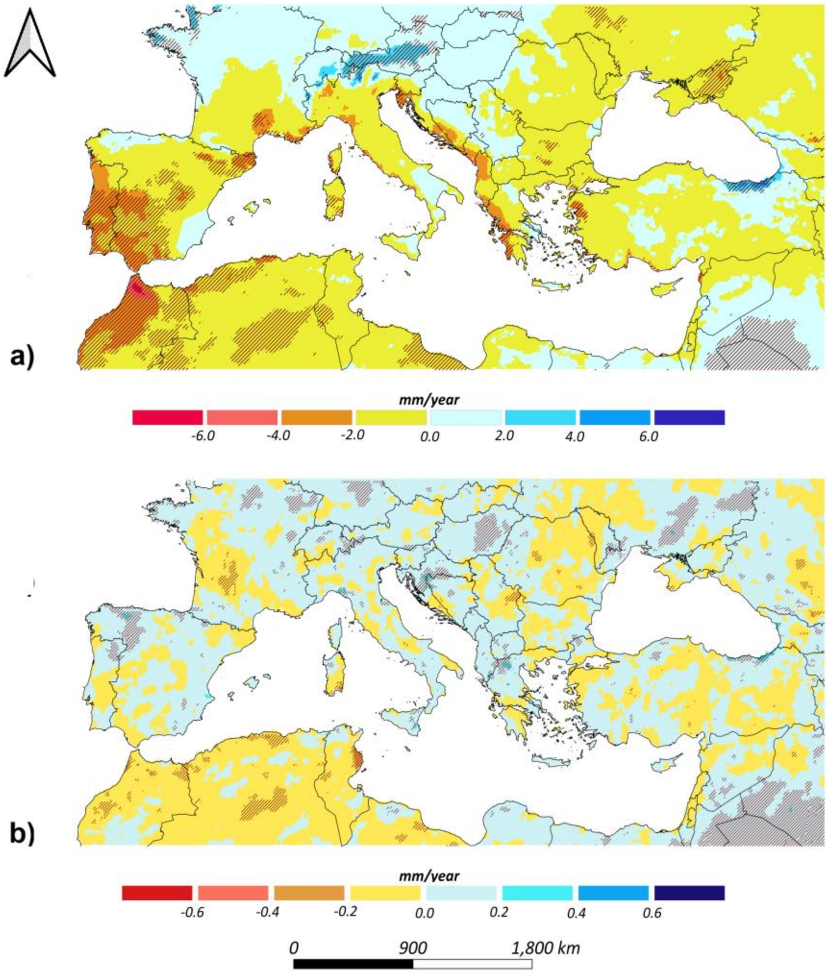


Fig. 3: Mann-Kendall and Sen's slope (mm/year) tests for PRCPTOT (a) and RX1day (b) over the Mediterranean area (please refer to the revised paper for more details).

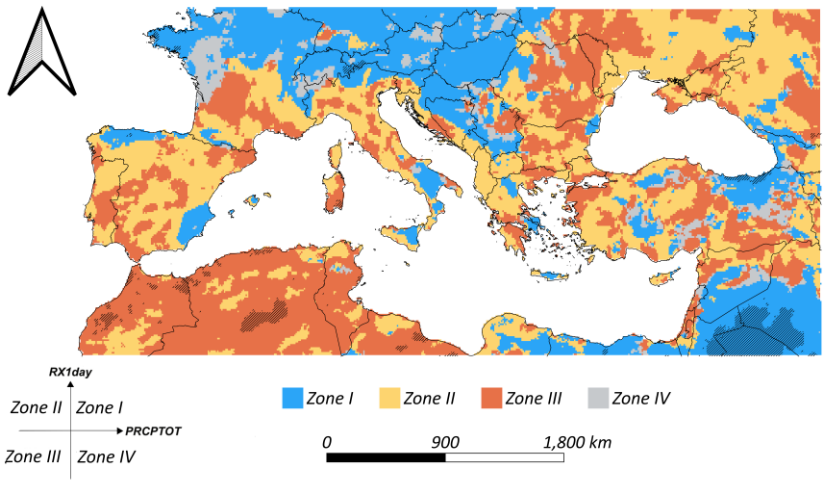


Fig. 4: Four zones of compound trends of PRCPTOT and RX1day at an annual scale over the Mediterranean region (please refer to the revised paper for more details).

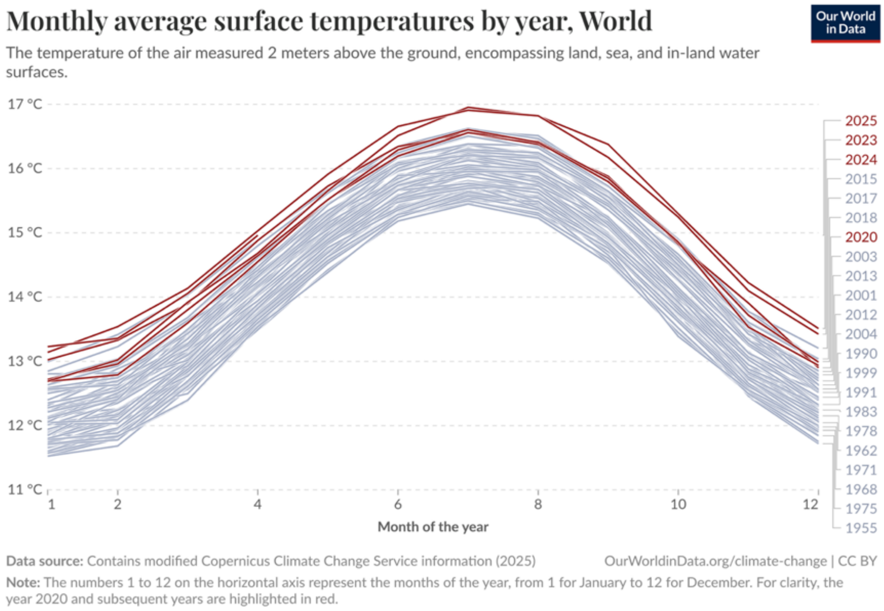
3) The Calabria region is at the focus of this study. The introduction lacks clear presentation and reader guidance about the region and why it serves as a case study, or why it is interesting to a broader readership unfamiliar with it.

Information highlighting the significance of Calabria in the regional context is added in the new Introduction in LL101-106. In addition, further details are provided in the subsection “Datasets and study area” (LL127-135).

4) The comparison of ERA5-Land and gauge data seems insufficient to conclude that ERA5-Land "can catch the contrasting daily and annual precipitation trends" as well as the "results achieved along the entire northern Mediterranean coast". This is because: (a) no direct comparison was made. Could you provide a direct comparison of the results? For example, a scatter plot of gauge vs. ERA5-Land trends. This could either be done on a gauge-pixel basis, or interpolated gauge-to-pixel-pixel basis. (b) The fact that results are promising over one area (Calabria) does not mean they are like that, or should be like that, over extensive areas such as the northern Mediterranean. If this claim is something you would like to present, please show a wider comparison using gauges from other areas of the northern Mediterranean as well. Daily gauge records are rather easy to find, and thus, this analysis seems manageable. Similar comparisons could be made with other, gridded datasets, considering their limitations (e.g., L228-239).

A direct comparison between ERA5-Land and ground observations (point (a) raised by the Referee) is proposed in LL293-307, supported by the Figs. S3 and S4 (supplementary). Concerning the general level of agreement between ERA5/ERA5-Land products and observations in the Mediterranean (point (b) raised by the Referee), we supported our statements with a more comprehensive bibliographic review (LL438-456). However, the previous (misleading) statements about the reliability of ERA5-Land in the Mediterranean context have been removed.

5) Sect. 3.2 and Figures 6 and 7: Given that there is no description of the use of specific CMIP models over specific months, in my view, the details provided in Figures 6 and 7 are excessive. You could provide, for example, a plot like the one below, showing the changes throughout the year. If the specific models are needed you could plot them as spaghettis, and if they are not needed, a shaded area would be more helpful. This could be done for the actual values and for the difference ("delta") values, and if it looks alright you could even provide the two SSP scenarios on the same plot, thus, minimising the figures to only two panels. More generally, it seems like the whole purpose of this section is to justify the use of the specific numbers which were then simulated, which is alright, but you could provide these numbers with a symbol on a graph (e.g., add the 2019 values and the -1, 0 and +3 values to the "delta" panel plot) and make the text much shorter (one paragraph).



We sincerely thank the Referee for this suggestion. Previous Figs. 6-7 have been replaced by new Fig. 8. Text in the subsection 3.2 ("Observed and projected SST warming") was changed accordingly.

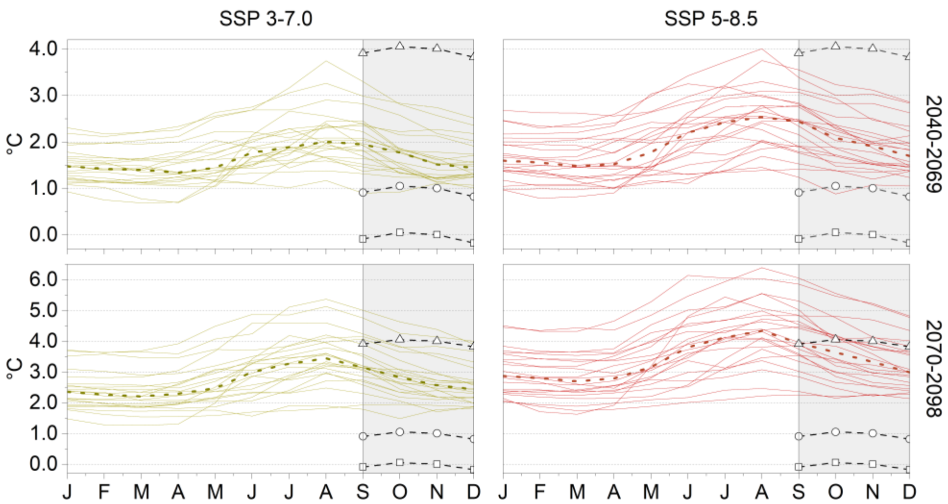


Fig. 8: SST increase compared to 1985-2014 in the periods 2040-2069 and 2070-2098, and the observed anomalies in 2019 compared to the reference period (please refer to the revised paper for more details).

6) WRF event simulation validation. In the FSS analysis, you use the 95th percentile, which is quite high, and get rather low values. While you mention that 0.5 "typically indicates valuable skill", actually it is 0.5 + the occurrence of half of a random forecast (Roberts and Lean, 2008), meaning that almost none of your forecasts are "useful" as termed by Roberts and Lean (2008). Even if we take the small portion of useful forecasts, they lie mainly in the larger averaging domains (e.g., "Size 19"), so the forecast is useful for 180X180 km and more, which is quite a bad statement. Please consider using the FSS with a lower threshold, unless you're specifically interested in pinpointing the really high precipitation accumulation values. You could, for example, take the 50th percentile out of the "rainy" pixels (e.g., where rainfall accumulation is greater than 1) or something similar. That's just an example for a threshold

that I see as valuable but other thresholds could also be used. The thing is that taking the 95th percentile means you are trying to forecast extreme values and you actually show that this prediction is bad. Additionally, when describing FSS and CSI scores for the SST-1 and SST+3 simulations you are showing validation of events which we do not expect to be similar to the observed. Therefore, the meaning of the validation of this forecast is vague. You can, however, suggest that lower scores indicate rainfall patterns had changed between the events, but this is sort of a weird way to examine it. A simpler way would be an application of a metric that measures similarity between precipitation fields, for example the SAL analysis (Wernli et al., 2008) where SST0 serves as the "truth" while each of the other scenarios is examined against it.

The FSS index was recalculated using the 90th percentile on the cumulative precipitation observed as the threshold (L220). New results are presented in LL346-356, and in the new Fig. 10a. We decided to keep the comparison with SST-1 and SST+3 scores, to highlight the extent to which SST representation affects the simulations, as explained in LL353-354. The SAL analysis was added to the new Fig. 12 to emphasize the effect of the SST scenarios and discussed in LL408-411.

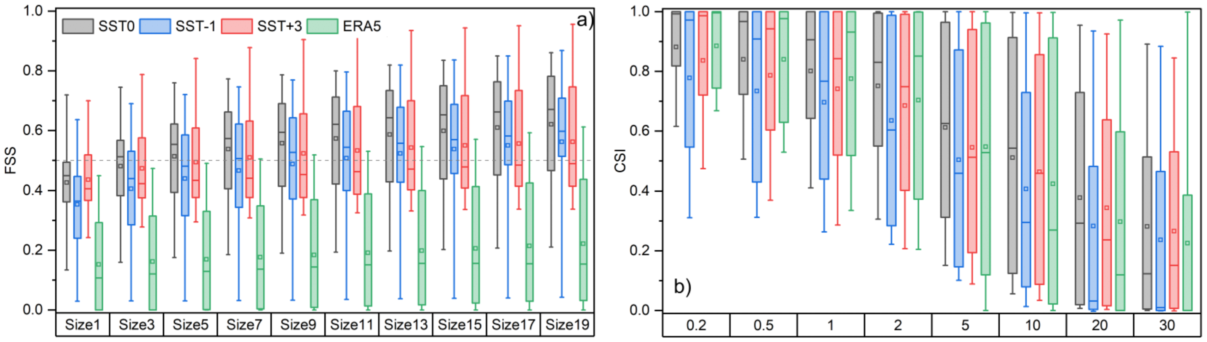


Fig. 10: Box and whisker plots of the Fractional Skill Score (a) and the Critical Success Index (b) (please refer to the revised paper for more details).

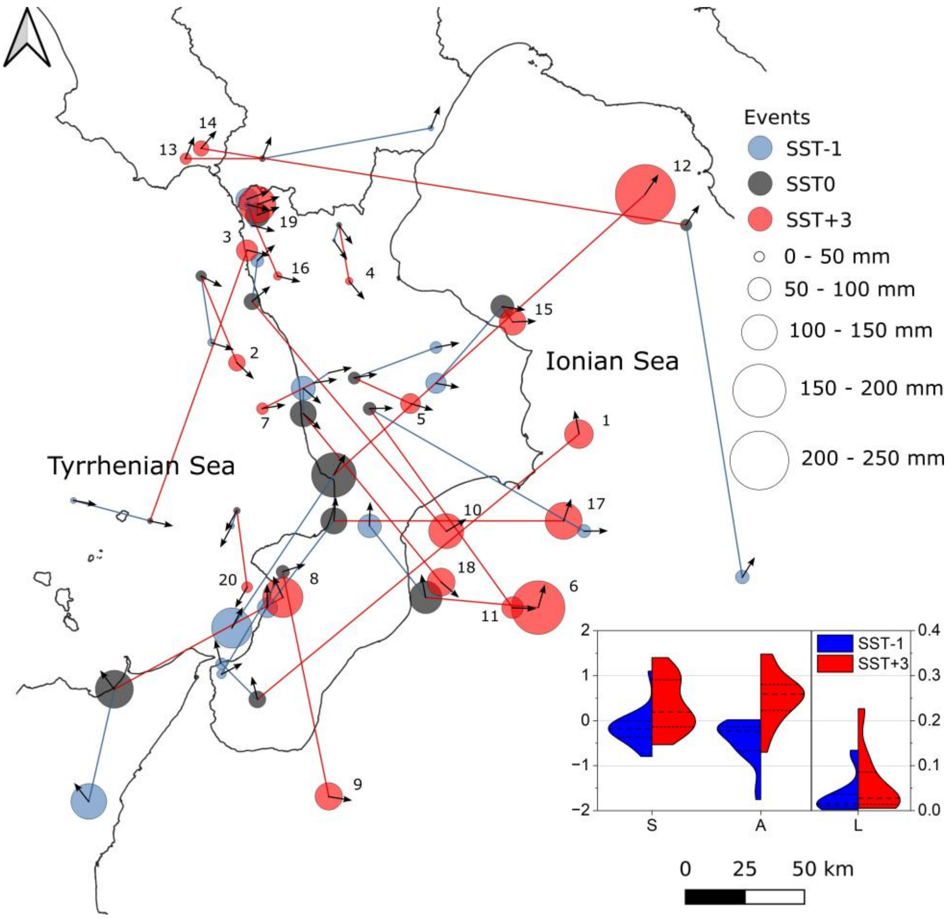


Fig. 12: Locations of the centers of mass of the accumulated precipitation exceeding the 95th percentile for the 20 analyzed events and the SAL violin plot (please refer to the revised paper for more details).

7) L327–337 and Fig. 13: Event #12 shows quite a lot of precipitation over the Tyrrhenian Sea and even more over the Calabrian peninsula. From the accumulated precipitation map (Fig. S12) it seems like there is a significant orographic enhancement of rainfall during the event. At the same time, it seems like there is quite a low correlation between the CAPE values presented (from the night before the peak of the event) to the accumulated precipitation map. This requires elaboration: either the night before is not a good representative time step, or CAPE is not such an important indicator for precipitation during the "actual" event. If the latter is the case, why should CAPE be a good indicator for the changes occurring between the three different scenarios? Did you consider examining the moisture flux perpendicular to the coastline/mountain ranges? Could you think of other mechanisms explaining this shift towards the Ionian Sea? How about the rainfall across the entire domain, does it go up or down in this event? A graph would help, so please consider adding another panel to Fig. 11 with precipitation over the entire domain. Can this shift be arbitrary? From Fig. 12 it seems like the shift is consistent, but the explanation is not clear to me. Can it be related to a shift of the cyclone center toward the Ionian Sea? If so, why? In my view, to better explain this part, a more detailed analysis is needed. Last note about this topic, "Negative values of omega, related to vorticity advection (e.g., Lenderink et al (2017))" — such values are not necessarily the result of vorticity advection, and are important indicator for precipitation even without vorticity advection. Also, if I remember correctly, the vorticity advection topic is not discussed in Lenderink et al (2017).

The Results subsection 3.3 (“High-resolution atmospheric simulations”) was largely revised. Fig. 11 was modified according to the Referee’s comments.

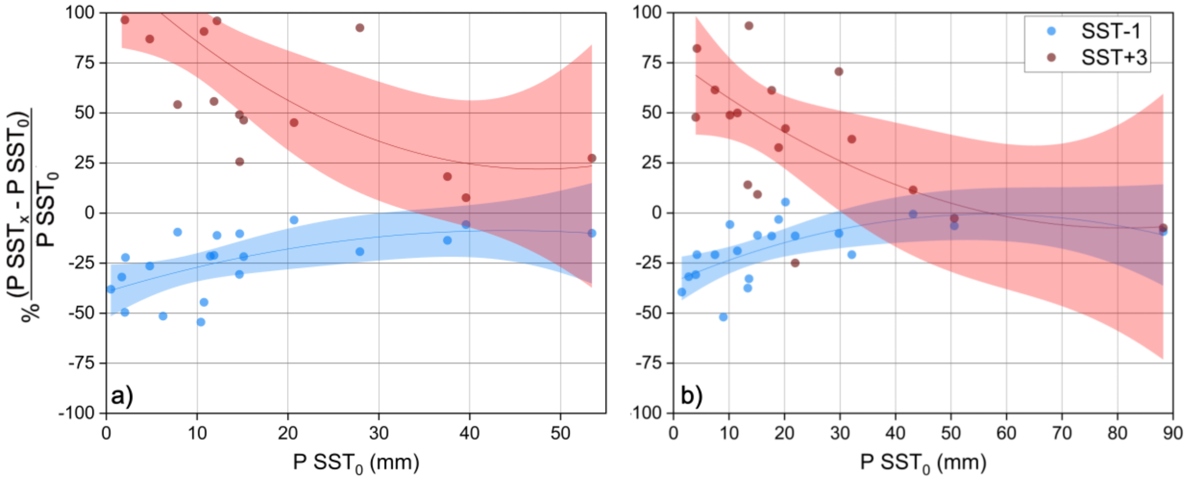


Fig. 11: Percentage variation of the accumulated (spatially-averaged) precipitation all over the D02 domain (a) and only overland (b) (please refer to the revised paper for more details).

The underlying mechanisms driving the eastward shift of maximum precipitation accumulation are now described in more detail in LL401-407. A SAL analysis was added (LL408-411) to confirm the results achieved. Furthermore, the analysis of the single event (event no. 12) was extended (LL412-435), adding two new figures (Fig. 13 about synoptic configuration and Fig. 15 about vertically integrated water vapour flux across a specific section), and complemented with the analysis of two other events (no. 6 and no. 15, Figs. S27-S29). We kept the CAPE graphs (Fig. 14 and Figs. S28-S29) since we believe that they effectively highlight both the thermodynamical and dynamical aspects of our simulation results, emphasizing the increasing atmospheric instability over the Ionian Sea, with the humid air mass coming from the south and being fed by the increasingly warmer sea surface, changing significantly the cyclone track and the spatial pattern of precipitation.

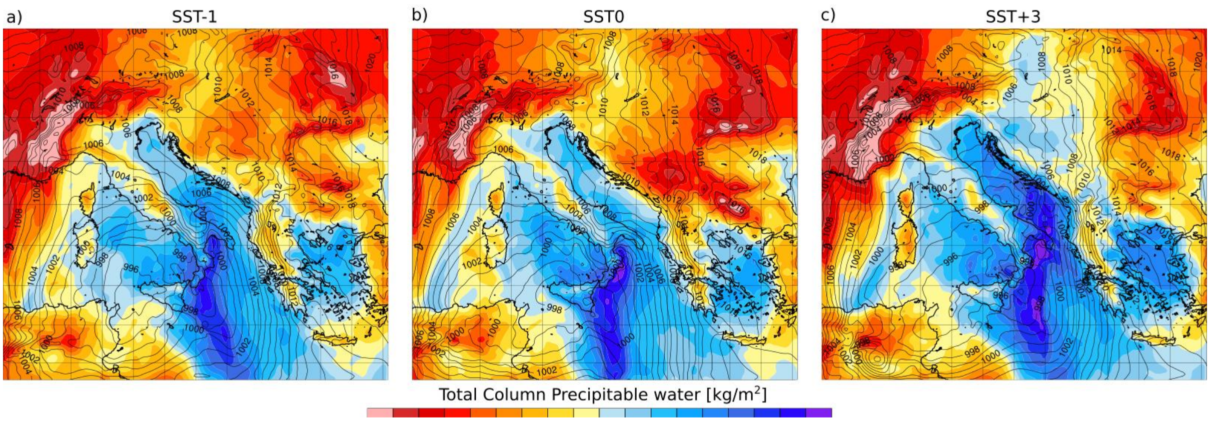


Fig. 13: Column precipitable water (kg m^{-2}) for configurations a) SST-1, b) SST0, and c) SST+3 and event no. 12 (please refer to the revised paper for more details).

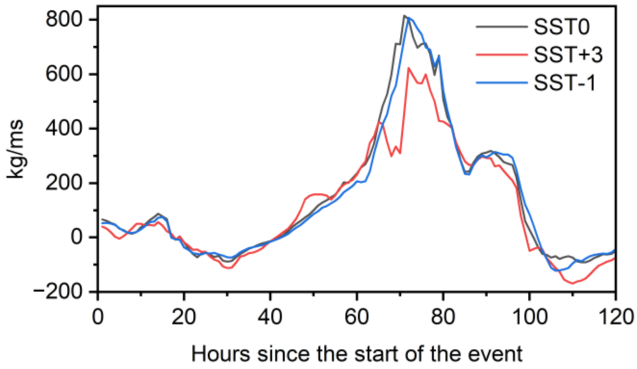


Fig. 15: Temporal evolution of the vertically integrated water vapor flux (kg/ms), perpendicular to section A-A' shown in Figure 1a, during event 12.

Finally, regarding omega, we agree with the reviewer that the phrase "related to vorticity advection" is misleading and was removed. Additionally, we agree that the reference to Lenderink et al. (2017) is incorrect in this context.

8) Increase in extreme precipitation despite overall drying. The authors claim to investigate the intensification of heavy precipitation in the context of overall drying over the Mediterranean. However, the analysis seems to show that precipitation is increased across almost all rain events within the examined season in SST+3 and the opposite for SST-1. This raises questions about the apparent contradiction between their results and the broader drying trend they show and refer to. I would like to see some discussion explaining why this is the case and whether the results actually show the intensification of extremes, or rather they show a general intensification which is related to rising SST. If the latter is the case, this should be explained, and the discussion could describe the contradiction by suggesting alternative reasons for the overall drying, like reduced cyclone frequency (Zappa et al., 2015), shortening of the rain season (Hochman et al., 2018), decreasing land-sea gradients (Tuel and Eltahir, 2020) and a decrease in the area and duration of rain events (Armon et al., 2022).

As suggested by the Referee, the Discussion was improved, adding the suggested literature (LL499-512, and L514-517).

Specific comments

L18-L28: The first paragraph is very general on the one hand, but puts a lot of emphasis on the expansion of the Hadley Cell on the other hand. While this is nice as a general introduction, it does not fully lead the reader to a better understanding of the background related to this specific study. I would suggest either being more specific — i.e. what is special about the climate change of the Mediterranean, or adding other examples to what governs climate change effects over the region in addition to the expansion of the Hadley Cell.

While we kept the issue of the expansion of the Hadley Cell, we provided a more specific introduction about climate change issues in the Mediterranean Basin (LL28-36).

L34-36: The claim for increased cyclonic activity is **not supported** by the cited reference. Aragao and Porcu's (2022) claim is that their algorithm produces 40% more cyclones compared with other cyclone detection algorithms. Please revise. Please correct also the connection to medicanes. Medicanes are indeed formed in the Mediterranean and lead to destruction, but this is not related to the fact the number of cyclones in the Mediterranean is increasing (which, in any case, is not supported by the reference cited).

We thank the Referee for this very timely comment. The reference to Aragao and Porcù (2022) was removed, and the entire sentence rewritten (LL43-45).

L105-106: Did the authors apply the mentioned techniques? If so, would you please elaborate? If not, please describe better the procedure and mention who is responsible for it. Could this procedure change the value of extreme events?

We used linear regression. A detailed description is now provided in LL137-143. While this approach could influence extreme-event values (but not the recorded and validated ones), we applied it cautiously, taking into account the characteristics of the data.

L123: To my view, there is not much of a comprehensive overview of the dataset in Table 1. Rather, there is a list. Please delete the text in the parentheses except for the words "Table 1".

Done (L166).

L133-134: " Non-parametric trend tests like the... were employed" — please be specific. Are these the only tests or are there others?

We used only these tests. The sentence was modified to make the text clearer (LL176-179).

L195 (and L208): " resulting in more probable flooding challenges across Europe (Fig. 2b)." This claim is problematic since greater RX1day does not necessarily mean more probable flooding challenges, as floods come in different flavours. See for example Bloschl et al. (2019).

The cited sentence in the previous L195 was removed. The sentence in the previous L208 was smoothed (now “which could contribute to higher flood risk”, L270).

L196: " These results are largely consistent with previous literature." Is there literature showing ERA5-land trends over the EURO-CORDEX region? If there is, this should have been mentioned in the introduction. If there is not, it is better to explicitly mention the "previous literature". In that case, the introduction misses a description of what was not done before which you are showing here (e.g., calculating trend in ERA5-Land versus trends in other models).

We referred to previous literature regarding the trends achieved, either by using or not using ERA5-Land. We considered the reference to the 6th IPCC Assessment Report sufficient since it is a sort of summary of many other works. Now, this discussion is further enhanced in LL457-467 (please note that, according to a suggestion from Anonymous Referee no. 3, we separated Results and Discussion and devoted to the Discussion a specific Section, i.e., Section no. 4). We feel that it is not the case to add a detailed discussion about calculating trend in ERA5-Land versus trends in other models in the Introduction, to avoid to make it even longer and more complicated. We only added the concept that we are going to "verify and update the information about the decreasing mean/increasing variance paradox" (LL96-97).

Figure 3: Could you add axes labels to the small quadrant graph? This would make it much easier to read (rather than remember) what every zone represents. A different approach would be to incorporate all zones into one graph but vary the colours. Could you try this approach in order to minimise the number of panels for this figure?

Previous Fig. 3 (now Fig. 4) was modified according to the Referee's comments. Please refer to our reply to the major comment no. 2).

Figure 4: The resolution of the figure is too low. Please provide a better resolution version of it.

Previous Fig. 4 (now Fig. 5) was modified and its resolution increased.

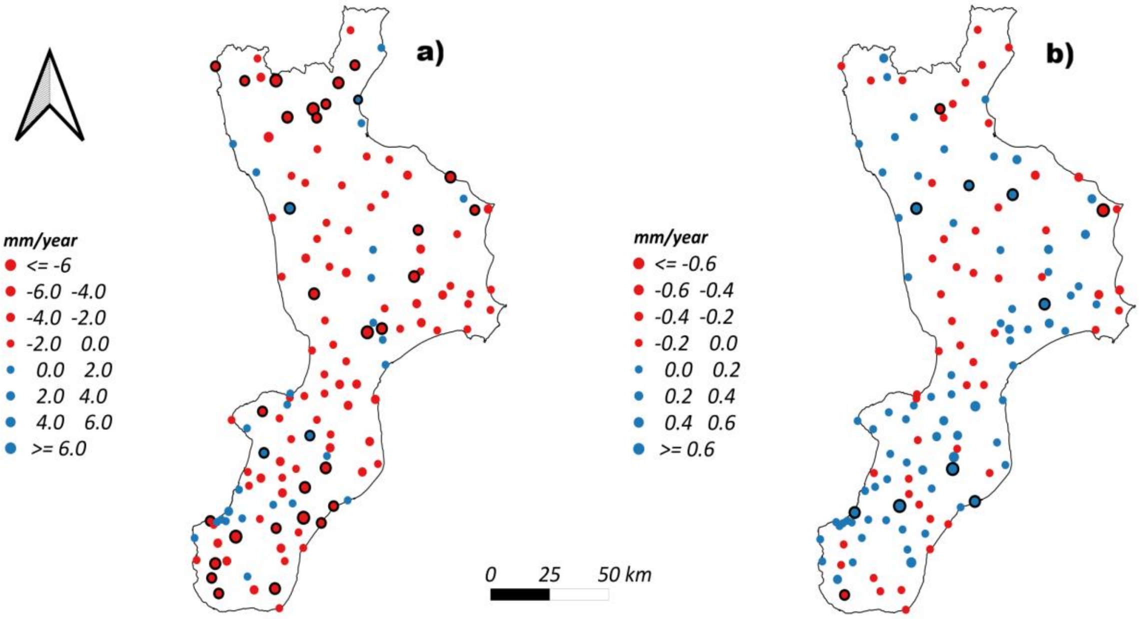


Fig. 5: Mann-Kendall and Sen's slope tests for observations of a) PRCPTOT, b) RX1day over Calabria from 1955 to 2023 (please refer to the revised paper for more details).

L213-218: The results of significant trends in RX1day should be treated carefully! Considering random processes, we would expect to find more or less 7 gauges (5%) with positive and 7 gauges with negative trends. Please make sure to address this. In contrast, L219-227 discuss mainly the non-significant trends. There, a note should be highlighted saying that these are non-statistically-significant results.

A discussion about the non-significance of many of the detected trends is provided in LL468-478 (Discussion subsection 4.1). However, the fact that few stations have significant trends, especially concerning RX1day, is already highlighted in LL274-276.

Figure 5: I would suggest the same as in Fig. 3 — combine all quadrants and vary the colours.

Previous Fig. 5 (now Fig. 6) was modified according to the Referee's suggestion.

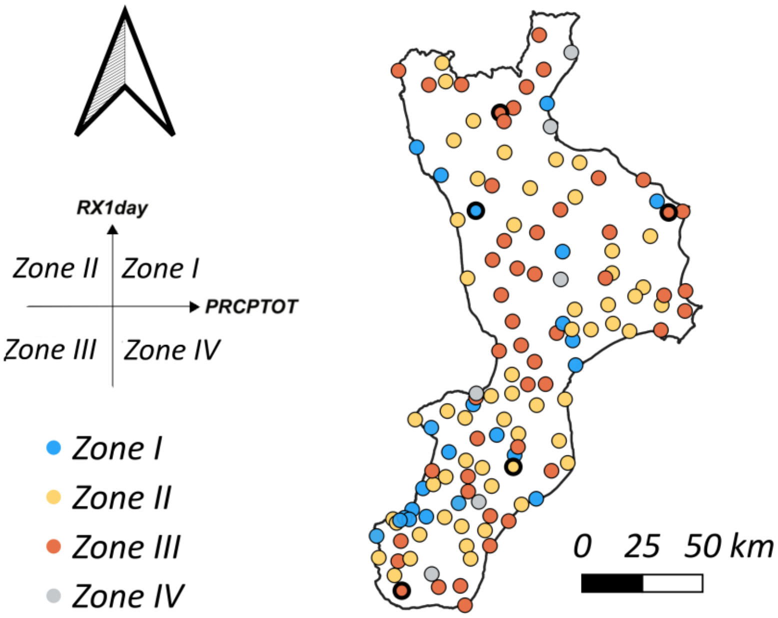


Fig. 6: Four zones of compound trends of PRCPTOT and RX1day at the annual scale over Calabria from 1955 to 2023 (please refer to the revised paper for more details).

L240-241: This claim is not clear to me. Please explain.

In accordance with our reply to the general comment (b), the statement has been revised and further clarified in the manuscript (LL309-312).

L254: Please explain what is the value which the "±" sign refers to (is this the range, standard deviation, 10–90 quantile range?)

The values reported with the symbol ± represent the standard deviations for both the SSP3-7.0 and the SSP5-8.5 scenarios. We have clarified this in the revised manuscript (L327).

Figure 9 and L276–277: Could you provide a reference to another figure, preferably Fig. 1, where you show the area over which you do this spatial interpolation and its boundaries? Is it the same area for the observations and model? What type of interpolation are you using?

We interpolated over the Calabria region boundaries, shown in Fig. 1a (in the caption, we specify that the administrative borders are drawn), using the 150 rain gauge stations highlighted. The description of the methodology used for spatial interpolation is given in LL151-153 and involves IDW (Inverse Distance Weight). Of course, the area compared is the same for the observations and model. We reiterated this in the figure caption.

Additionally, some events seem to have a less skilful simulation, especially in terms of the spatial distribution of rainfall, for example, event #2. Could you provide some details why that is the case while others are simulated almost perfectly?

If we have understood the question correctly, a detailed answer would likely exceed the scope of this paper. In this context, we can provide some general reasons, including the correctness of the boundary conditions, the difficult predictability of local convective events, and the overestimation of the orographic effect by WRF simulations in certain situations (in the case of event #2, it seems to occur in the central-southern part of the region). Furthermore, in the case of event #2, the Calabria region was at the very border of the transit of a cold front. In any case, we prefer not to distract the reader from the paper's main topic.

Figure 10. If I understood you correctly, boxplots represent the variability across the 20 different events. Is that correct? Please describe this in the text. Furthermore, the labels "Size1... Size19" are not clear. Is this when averaging across 1 pixel, 3 pixels... 19 pixels? Please explain.

Yes, the boxplots represent the variability of the 20 different events. More details are provided in the revised version of the caption.

Figure 11. What is symbolised by the shaded area? What is represented by the line? Please explain. Also, could you add a panel showing the (non-normalised) linear regressions discussed in L299–L310? Additionally, when presenting values in a normalised axis, it is common to present it in log distances, such that e.g., a decrease of 50% has the same distance from 0 change as an increase of 100%.

The shaded area represents the 95th percentile confidence level performed for the 2nd-order polynomial fitting (i.e., the line) over the two different scenarios. We specified this point in the revised caption. In previous LL299-310, only a couple of linear regressions were discussed (L300). Probably the Referee also refers to the other two couples of regressions (previous L297 and L318). We feel that this panel could be redundant, given Table A1, and prefer to avoid adding it. Regarding the representation in log distances, we've made various attempts to improve the visual format of the graph and concluded that log distances do not add any benefit (i.e., they do not highlight any specific feature) while making the graph less readable.

L342–343: Could you explicitly write what you mean by those features? The authors' analysis is focused on precipitation accumulation, rather than intensities, and " tracking" is not clear to me in this context. "Cyclone tracks" is further mentioned in L401, which is again, not clear to me.

We agree that the terms used may be misleading and imprecise in this context. The sentence was modified ("[…] highlighting variations in features like average and maximum accumulation precipitation values and overall spatial patterns", LL482-483). Also, the term "cyclone tracks" in previous L401 was removed ("cyclone features", L555).

L355–357: Similar conclusions were made earlier by Zappa et al. (2015). Please consider citing them.

The reference was added in the revised manuscript according to the Referee's suggestion (L514).

L387: For Storm Daniel, please consider using more relevant literature focusing on the meteorology and hydrology of the event, e.g., Armon et al., (2025) or Flaounas et al. (2024).

The references were added in the revised manuscript according to the Referee's suggestion (L49).

L405–407: This was shown before, over many different studies (e.g., Prein et al., 2015; Ban et al., 2014; Pichelli et al., 2021; Coppola et al., 2020).

The highlighted sentence was modified, considering both the Referee's comment and the fact that the topic remains highly debated. Furthermore, we noticed that the main features of precipitation change can be observed already by examining the outer (lower resolution) domain. The whole paragraph that included this sentence was revised (L559-566).

L411: Since many studies have already used PGW with more parameters changed rather than only SST, this sentence is not clear to me. Please consider revising this statement. Also, the statement in L413–415 is not in place here; it should either go in the discussion or be deleted, because other factors are competing with it, such as soil drying because of longer dry spells.

Especially the second part of the conclusions was deeply revised. The sentence in the previous L411 was recontextualized, linking it to uncertainty assessment. Furthermore, the need to link our findings to hydrological models to achieve a better impact assessment was presented in a different way. Please refer to the whole paragraph in LL575-587.

Data availability: The netcdf files contain accumulated precipitation, but the 'Times' (at least for the SST+3) vector is corrupted (it contains '0' only). Additionally, there is no spatial information except for the number of grid cells. If you can, it would be very helpful to accommodate the real times in the vector and add information about the coordinates of the data, e.g., lat/lon vectors or arrays.

We thank the Referee for noting that. The time strings and the lat/lon arrays have been uploaded to the Zenodo repository (a new link is provided).

Technical corrections

L40: "contributes to lead" — please stick with either contributes or lead.
Modified, we used "leads" (L53).

L42: "trigger dynamically" — please revise. You could, e.g. use "dynamically interact with orographic lifting" or something similar.
Modified (L55).

L60: "as the Ianos cyclone occurred in 2020, producing" would be more readable if you expand the sentence like "such as cyclone Ianos that occurred in 2020, which produced..."
Modified (L74).

L76: "events occurred" please expand to "events that occurred".
Modified (L108).

L97: "lies" should be "that lies".
Modified (LL127-128).

L295: "evapotranspiration" should probably be "evaporation".
Modified (L366).

Fig. 12: Please correct the "Tyrrenian" label in Fig. 12 to be similar to what's written in the text i.e., "Tyrrhenian".
Modified.

Anonymous Referee #3

This is an interesting study on understanding better the precipitation paradox in the Mediterranean (decreased mean precipitation/increased extremes). It falls within the scope of HESS, and I believe it could attract the interest of the scientific community. While there are different approaches and sound datasets used, there are no straightforward linkages between the various parts of the analysis.

We thank the Referee for the positive feedback and acknowledge that further effort is needed to harmonize different parts of the analysis (and of the manuscript). Below, we attempt to address the suggested concerns and comments.

1. The overall presentation is well structured and clear. One exception is the inclusion of discussions in the results. I would include any discussion in a separate section or present it with the conclusions. High-quality visualizations are used for the presentation of results. Some more effort could be put into making the language style more fluent.

We removed discussions from the Result section no. 3 and created a new Discussion section no. 4, divided into two subsections (4.1 – Trend analysis and 4.2 – Mediterranean Sea warming effect on precipitation), in which, besides moving already existing discussion paragraphs, new comments were added.

2. In the title, please mention that you refer to the “Mediterranean Sea Warming”.

We agree with the Referee’s comment and modified the title accordingly.

3. Overall, the introduction section is informative, however, the list of references is not exhaustive. Topics such as future extreme precipitation trends in the Mediterranean or why the region is characterised as a climate change hotspot could be better covered. Any research gaps and main objectives of the present analysis could be more emphasized.

We provided a more specific introduction about climate change issues in the Mediterranean Basin in LL28-36. In general, the Introduction was largely revised. Specifically, we focus on existing work on the SST effect on precipitation in the Mediterranean in LL69-78 and (new text) LL79-86. Then, we highlight the research gap in LL90-93 and improve the description of the object and the task of the paper in LL94-116, highlighting the peculiarities (strengths) of the analysis performed.

4. EURO-CORDEX is mentioned several times in the text, however, the only linkage with this regional initiative is the selection of the domain. Please mention this only once in the definition of your domain of analysis.

Following a comment from the Anonymous Referee # 2, the study area used for the regional analysis was reduced to the Mediterranean area (30° to 50° N and 15° W to 45° E). The text was changed throughout the manuscript, especially concerning the Results subsection 3.1 (“Trend Analysis”), where we provide the new Figs. 3 and 4. Therefore, the term EURO-CORDEX is now mentioned only when presenting the ERA5-Land dataset (L98).

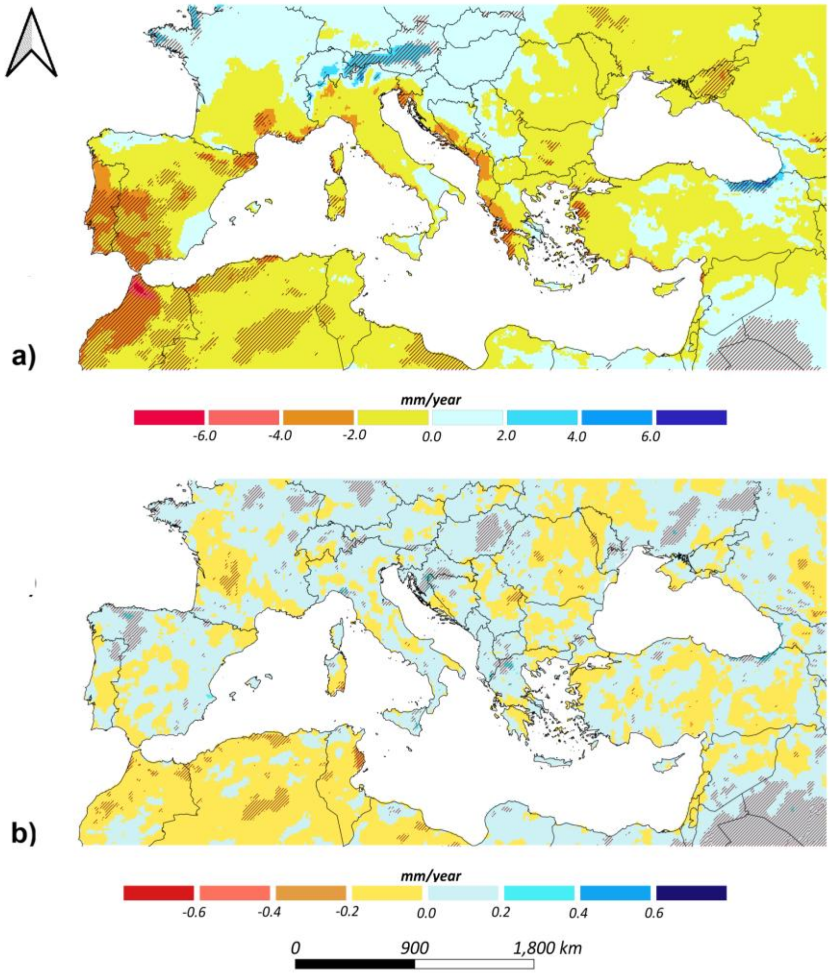


Fig. 3: Mann-Kendall and Sen’s slope (mm/year) tests for PRCPTOT (a) and RX1day (b) over the Mediterranean area (please refer to the revised paper for more details).

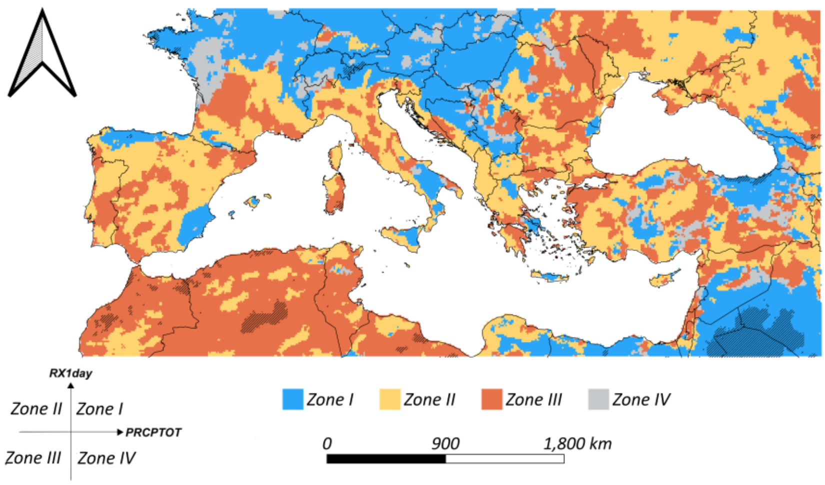


Fig. 4: Four zones of compound trends of PRCPTOT and RX1day at an annual scale over the Mediterranean region (please refer to the revised paper for more details).

5. Since the findings presented in Figures 7 and 8 are mostly used for estimating a reasonable warmer-SST scenario, I strongly recommend moving these two figures to the Appendix. The number of visualizations is already large.

Previous Figs. 6-7 have been replaced by new Fig. 8. Text in the subsection 3.2 ("Observed and projected SST warming") was changed accordingly.

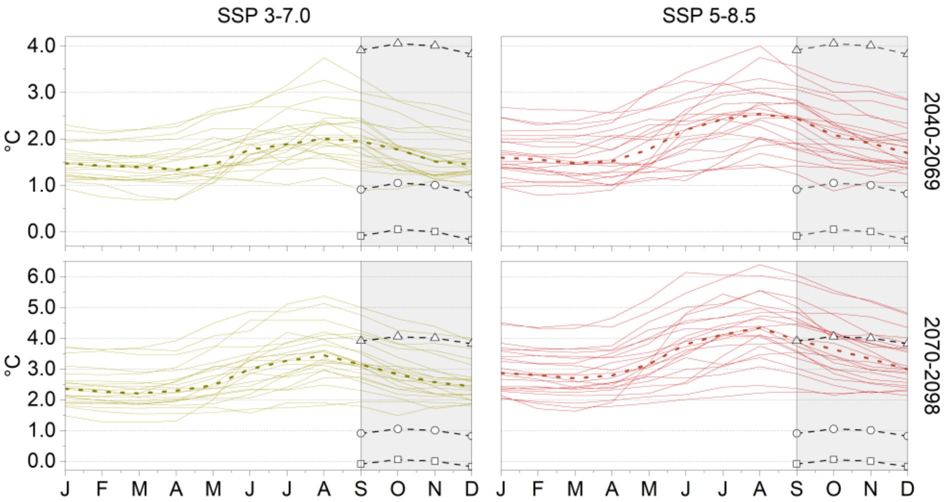


Fig. 8: SST increase compared to 1985-2014 in the periods 2040-2069 and 2070-2098, and the observed anomalies in 2019 compared to the reference period (please refer to the revised paper for more details).

6. In the methods section, it is not clear how future SSTs were taken into account in the WRF simulations. Some information is presented in the results (L262-269), however, this approach should be demonstrated in more detail.

The Data and Methods section was also deeply revised. Specifically, the SST perturbation approach is more comprehensively explained in the new subsection 2.3 "WRF downscaling and validation", in LL203-212.

7. Some additional explanation of the methods used to derive Figure 12 should also be included in the methods.

A revised explanation of the procedure followed to derive the positions of the barycentres of the areas where the accumulated precipitation exceeded the 95th percentile is provided in subsection 2.4 (LL235-244). Furthermore, Fig. 12 was improved by adding arrows indicating predominant wind directions at 850 hPa during the events and a SAL violin plot.

8. For increased confidence, I strongly recommend repeating the analysis of Figure 13 for an additional event. For example, for event 15, which is characterised by extreme rainfall, underestimated by the SST0 simulation.

The analysis of event no. 12 was extended (LL412-435), adding two new figures (Fig. 13 about synoptic configuration and Fig. 15 about vertically integrated water vapour flux across a specific section), and complemented with the analysis of events no. 6 and no. 15 (LL417-419, LL429-430, Figs. S27-S29).

Minor comments (provided in the attached PDF document).

comment #1 title modification
Done.

comment #2 degrees C per decade is a more common way of presenting temperature trends
Modified (L39).

comment #3 Please spell out numbers under 10
We carefully checked all the numbers under 10 and spelled them out.

comment #4 This should be defined here.
Done (LL65-66).

comment #5: “on”
Done (L69)..

comment #6: anomaly
done (L70).

comment #7: use plural precipitations
corrected (L70).

comment #8: remove some
Text changed (L72).

comment #9: Please be consistent in the use of units (degrees C or K)
We use °C here (L72) and all along the text.

comment #10: “such as in the case of”
Corrected (L74).

comment #11: remove “pan”
Done (L98).

comment #12: remove EURO-CORDEX
We decided to leave the term here according to our reply to major comment no. 4.

comment #13: modify “future scenario” to “an increased SST scenario”
The text was further modified (L113).

comment #14: remove the first sentence of section 2.1
Done.

comment #15: modify trend analysis to historical trend analysis
Done (L119).

comment #16: remove "of the European Commission"
Done.

comment #17: remove "Skamarock et al., 2021", the WRF extended name and "limited area model"
Done.

comment #18: Interesting way of presenting results, but should have been explained in methods
The explanation about the quadrant classification, already present in the Methods section in the previous version of the manuscript, has been improved (LL184-189). A diagram was also added, shown in the new Fig. 2.

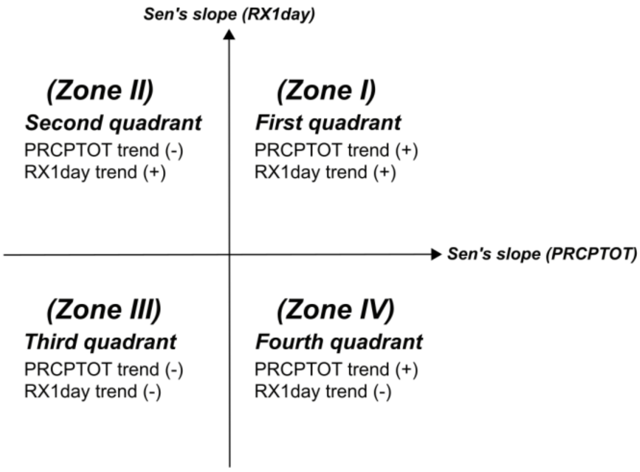


Fig. 2: Quadrant classification diagram based on Sen's slopes of PRCPTOT and RX1day.

comment #19: I assume that this is the case, but please clarify if these numbers and percentages refer to land-only grid cells.
The achieved results were obtained using ERA5-Land gridded data, which is limited to land areas. For the sake of clarity, we added the term "land pixels" (L263).

comment #20: I would include discussions in a separate section or merge with conclusions.
As we stated in our reply to comment no. 1, we devoted the new Section 4 to the discussion.

comment #21: This is not results material.
We agree with the Referee's comment and moved this paragraph to the 2.1 section "Dataset and study area" (LL144-149).

comment #22: modify "2" to "two"
Done (L146).

comment #23 (Figure 6 caption): modify "yearly" to "annual"
Done (caption of new Fig. 7).

comment #24: This is more of an Introduction material
We agree with the Referee's comment and moved this paragraph, modifying it, to the Introduction (LL47-51).

Anonymous Referee #4

In their study "Increasing Daily Extreme and Declining Annual Precipitation in Southern Europe: A Modeling Study on the Effects of Mediterranean Warming" the authors investigate how projected changes in SST would unfold with respect to precipitation extremes of the Mediterranean north shore and in particular for the region of Calabria. In addition they elaborate on the hypothesis that there is a general trend of decreasing total annual precipitation along with increasing daily maximums, based on an analysis of reanalysis data and local observations. The paper addresses an important topic and the rationale is reasonable. The analysis is sound and well structured. Some arguments and conclusions may require a deeper analysis and discussion than currently presented, also given the limited significance of the trend analysis.

We thank the referee for the positive feedback and acknowledge that some aspects of the analysis require further strengthening. Below, we attempt to address the suggested concerns and comments.

1. The analysis of the ERA5-Land data for the EURO-CORDEX area does show negative trends but they are only significant for the Iberian peninsula and some regions of northern Africa. The findings for RX1day are even weaker. This lack of robustness should be addressed in more detail in the discussion. From ERA5-Land it looks like for the Calabria region, most pixels are seen in zone I of Fig. 3 and only a few in zone II which would contradict your statement about the match of ERA5-Land and the local observations. If you skipped the non significant values in Fig. 5 only a few would remain. What's the reason for the non-significance? Are there recurrent outliers in the observations?

A discussion about the non-significance of many of the detected trends is provided in LL467-478 (Discussion subsection 4.1). However, the fact that few stations have significant trends, especially concerning RX1day, is already highlighted in LL274-276.
Regarding the behavior of ERA5-Land in the Calabria region, a direct comparison between ERA5-Land and ground observations is proposed in LL293-307, supported by the Figs. S3 and S4 (supplementary).

2. For the simulations with the regional atmospheric model it is assumed that just the SSTs are changing according to certain SSP scenarios. The atmospheric properties remain unchanged which creates an inconsistency for the described future conditions. In your WRF configuration, the GHG settings seem to be constant across your simulations. Later WRF versions, e.g., with the CAM radiation scheme allow for an adjustment of GHG concentrations and also support different SSPs. With these updated atmospheric settings, you should obtain a more realistic interplay between sea surface and the atmospheric boundary layer, mostly due to radiation effects.

The reviewer is correct that, since version 4.4, WRF allows for annual variations in GHG concentrations based on different scenarios (e.g., SSPs, RCPs, or A1B/A2 Scenarios). In the present study, our experimental design, which employed WRF V4.1, did not include adjustments to GHG concentrations. However, this limitation is not relevant for our scopes, since the main aim of our paper is to investigate the extent to which Mediterranean Sea

warming contributes to the seemingly counterintuitive increase in daily precipitation extremes in southern Europe, despite a general decline in annual precipitation. Our scientific objectives lie in a robust "research line aimed at disentangling the expected influence of sea surface warming, even combined with orography, on cyclonic events features in Southern Europe" (LL480-481). Therefore, adjusting GHG concentrations according to specific SSPs goes beyond our scope. Incidentally, making this choice would inherently imply the prior choice of a particular scenario and time period, which is not our intention.
We focus on existing work on the SST effect on precipitation in the Mediterranean in LL69-78 and (new text) LL79-86. Then, we highlight the research gap (i.e., why our research is needed) in LL90-93 and improve the description of the object and the task in LL94-116, highlighting the peculiarities (strengths) of the analysis performed. Finally, a discussion about the limits of our approach is proposed in LL499-512 (and in the discussion already present in the previous version of the paper - now, LL513-543).

3. Another concern is the influence of large scale dynamics and patterns. From the text it is not clear whether you applied the updated SSTs to both domains. I assume you did so also for the outer domain. Adding this much energy to the full extent of domain one could considerably change the larger scale dynamics, patterns and feedback. Therefore, to corroborate your findings of translocated precipitation events, I recommend to create another set of simulations to apply some spectral nudging to the outer domain, at least for the geopotential, to ensure consistency of the large scale structures also with respect to ERA5.

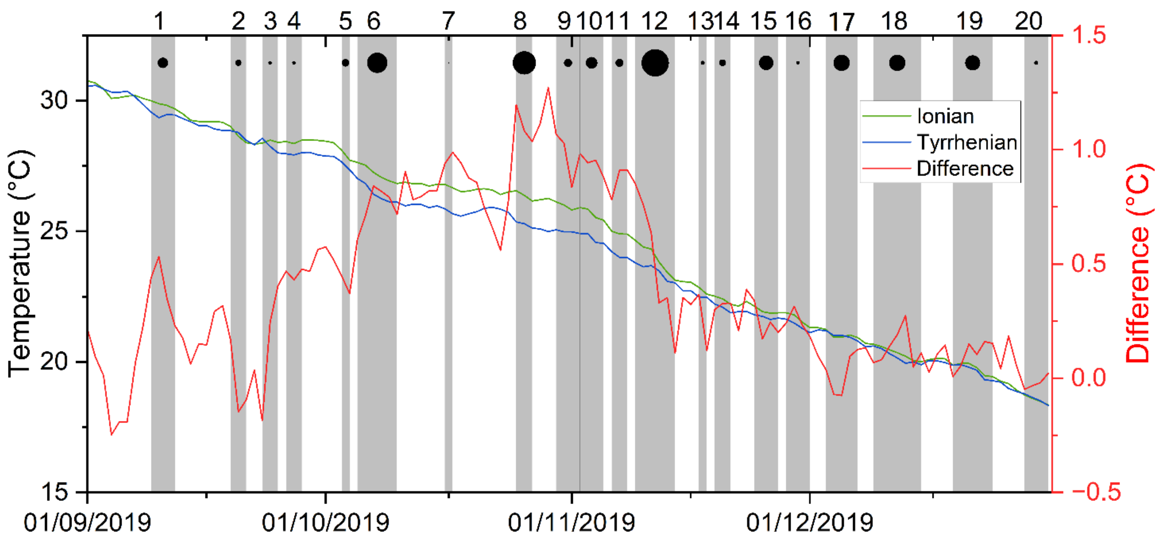
We uniformly applied temperature changes (+3 °C and -1 °C) to the SST fields on both calculation domains, including the outer domain. This aspect was better specified in LL203-207.
Concerning spectral nudging, following the Referee's suggestions, we performed two new experiments, both applying spectral nudging to the outer domain (D01) on the geopotential (ERA5 source) at heights above 500 m, for SST0 and SST+3 scenarios. While precipitation fields undergo slight changes, the effect of nudging is almost null concerning the extent of the eastward shift in precipitation, therefore confirming the robustness of our previous analysis. Results are described in LL207-212 and shown in Fig. S1.

4. Moreover, the effect of sea surface salinity should also be considered in your discussion or limitations section. Would the increase in SST and a decrease in total precipitation lead to increased salinity levels and how could that potentially diminish evaporation and consequently reduce severe precipitation events?

The discussion about the effect of sea surface salinity was added in LL506-508.

5. How relevant are gradients between the SSTs of the Tyrrhenian and Ionian Sea for the emergence of extreme precipitation events in the region?

We analyzed the SST behavior in the innermost domain D02 to detect possible connections with precipitation events, considering the SST0 scenarios. The figure below shows the daily SST evolution in the Tyrrhenian Sea (blue) and the Ionian Sea (green), along with their difference (red). Moreover, we highlighted the 20 events, and the bubble graph is proportional to the average precipitation simulated for each event.



The results highlight that the Ionian Sea is warmer than the Tyrrhenian Sea (111 days out of 122), in agreement with literature (e.g., García-Monteiro et al., 2022). Focusing on the connection with the precipitation events, the results show that almost 19 events were characterized by an averaged warmer Ionian Sea, with only event 20 as an exception. However, we did not find a direct and significant correlation between the intensity of the SST gradient and the intensity of the simulated precipitation. The relationship appears inconsistent across events. For example, in event 12, the event with the highest accumulated precipitation, the SST gradient decreases during the event. This decrease is likely an effect of the event, rather than its cause, since the cyclone primarily crossed the southern Ionian coast. Event 6, by contrast, displays an increasing gradient during the event, similar to most of events 17 and 19.

While the SST gradient between the two seas remains a relevant factor, and it could be connected to the general development of the cyclone at the mesoscale, our experiments involved a uniform adjustment of SST fields, making the gradient's role comparatively less significant in these cases. Thus, we prefer not to add this analysis to the revised version of the manuscript, to make the latter more straightforward.

García-Monteiro, S., Sobrino, J. A., Julien, Y., Sòria, G., & Skokovic, D. (2022). Surface Temperature trends in the Mediterranean Sea from MODIS data during years 2003–2019. *Regional Studies in Marine Science*, *49*, 102086.

6. How are the main horizontal wind fields for the precipitation events (Fig. 12) organized? Is there any obvious clustering for the big events, e.g. all originate from the south? Is it possible to annotate the dots of the events with an arrow that shows the direction of the storm path?

According to the Referee's suggestion, we added the direction of the storm paths in the new version of Fig. 12. Concerning general circulation, we provided a comprehensive study on weather patterns in the Mediterranean and their impact on the Calabria region (Mastrantonas et al., 2022). In the paper, we highlighted that the relocation effect of the accumulated rainfall peaks over the Ionian Sea "was found especially for the cyclonic circulations that cross the Ionian Sea before reaching the eastern Calabrian coast, which are, in general, the heaviest and most impactful" (LL391-393). Some other insight is also provided in the explanation of the underlying mechanisms driving the eastward shift (LL401-407).

7. Are the obtained spatial shifts of the event centers consistent for small perturbations (e.g. another PBL scheme or varied initial conditions)? Was there any spin-up performed to exclude impacts of imbalanced soil moisture?

About the spin-up: we performed single simulations from September 1st to December 31st. We did not consider the first potential event because it was simulated in the first week of the simulation, as can be observed from Fig. 9, where the event E1 is indeed the second event recorded in the study period.
A sensitivity analysis about PBL parameterization is proposed in LL397-400 and Fig. S26, which further confirms the robustness of the results achieved.

8. How do these extreme events look like in the outer domain: Are the centers of precipitation mass identically located to what was found for the inner domain?

A systematic analysis of all centers of mass for the external domain was performed, showing that the main features of the eastwards shift are also caught by the lower-resolution simulation (LL396-397 and Fig. S25).

Minor

9. The title is probably not so ideal since your main focus is on SST sensitivity. Maybe better: "… on the Effects of Mediterranean Sea Warming"?
We agree and modified the title accordingly.

10. L33: Give the actual years instead of "In the last two years"
Modified (L42).

11. L324: From Fig. 13 it seems that the storm system of event 12) travels along the coast in northward direction and that the high precipitation over the sea occurs after it traveled over the east part of the Calabrian coast rather than "exploding before".
This misleading sentence has been modified ("[…] so that the heaviest precipitation is induced offshore rather than overland", L396).

12. L391: I think the investigation should not be called "comprehensive" since many real-world aspects had been left out in this PGW experiment.
The term "comprehensive" was removed (L545).

13. Figure 2: add the term ERA5-Land to the caption.
Done (now, Fig. 3).

14. Figure 3: add the term ERA5-Land to the caption. Increase image resolution.
Done (now, Fig. 4). The figure was significantly modified compared to the previous version.

15. Figure 4: "Mann-Kendall and Sen's slope test for observations of a) ..."
Done (now, Fig. 5).

16. Figure 5: Increase image resolution. It would be good to scale the point size by the trend values. It's also hard to distinguish the significant values. A different color might be better to show them. Add "observations" to figure caption.

According to this comment and another from Anonymous Referee #2, previous Fig. 5 (now, Fig. 6) has been deeply modified. Also, the caption was modified as suggested.

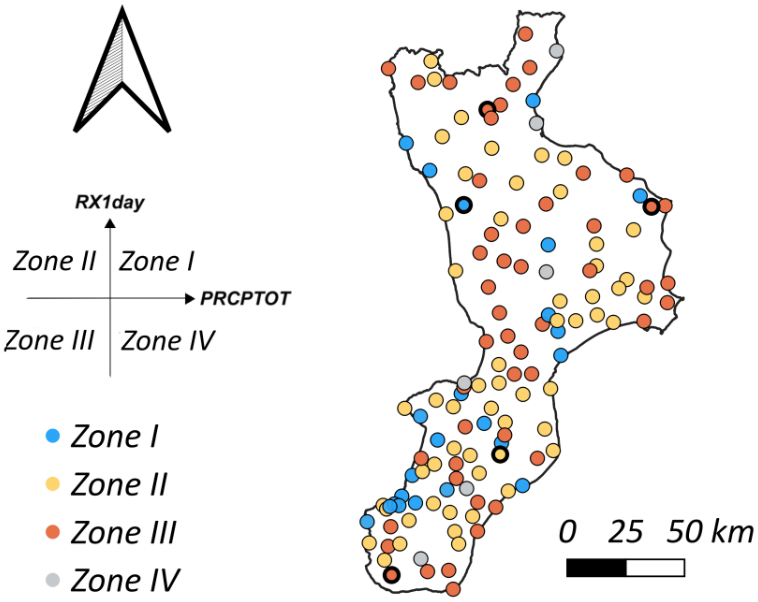


Fig. 6: Four zones of compound trends of PRCPTOT and RX1day at the annual scale over Calabria from 1955 to 2023 (please refer to the revised paper for more details).

17. Figure 7 & 8: annotate the periods in the figure and add "GCM ID" or similar to the y-axis.

Previous Figs. 6-7 have been replaced by new Fig. 8. Text in the subsection 3.2 ("Observed and projected SST warming") was changed accordingly.

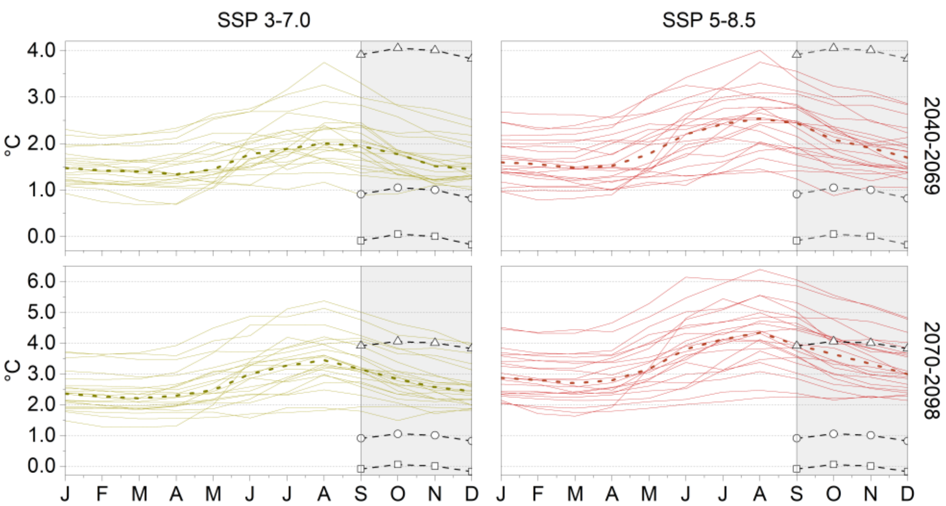


Fig. 8: SST increase compared to 1985-2014 in the periods 2040-2069 and 2070-2098, and the observed anomalies in 2019 compared to the reference period (please refer to the revised paper for more details).

18. Figure 10: Add "precipitation" somewhere in the figure caption
Done.

19. Figure 13: Add red cross section line also to c) and e) and add the center of mass points for event 12; what is the unit of Omega?
Done (Now, fig. 14). The same elements have been added to the new Figs. S28 and S29. Omega is formally defined as the variation of pressure over a time interval (hPa h^{-1}). The units have been added to the color bar legend.